LETTERS TO THE EDITOR.

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Change of Feeding Habits of Rhinoceros-birds in British East Africa.

THE enclosed extract from a letter just received by me from my friend, Captain Hinde, of the British East Africa Protectorate, will interest all zoologists. It is a curious fact that a bird which is so valuable as Buphaga in clearing parasitic insects from cattle that we lately agreed to give it special protection at the International Conference on the Preservation of African Wild Animals, should now, by a sudden change of conditions induced by man, become a dangerous and noxious creature. This fact shows how difficult is the problem presented by the relations of civilised man to a fauna and flora new to his influence. E. RAY LANKESTER.

Natural History Museum, London, August 10.

"The following case of wild birds changing their habits may interest you:—The common rhinoceros-bird (*Buphaga erythroepyncha*) here formerly fed on ticks and other parasites which infest game and domestic animals; occasionally, if an animal had a sore, the birds would probe the sore to such an extent that it sometimes killed the animal. Since the cattle plague destroyed the immense herds in Ukambani, and nearly all the sheep and goats were eaten during the late famine, the birds, deprived of their food, have become carnivorous, and now any domestic animal not constantly watched is killed by them. Perfectly healthy animals have their ears eaten down to the bone, holes torn in their backs and in the femoral regions. Native boys amuse themselves sometimes by shooting the birds on the cattle with arrows, the points of which are passed through a piece of wood or ivory for about half an inch, so if the animal is struck instead of the bird no harm is done. The few thus killed do not seem in any way to affect the numbers of these pests. On my own animals, when a hole has been dug, I put in iodoform powder, and that particular wound is generally avoided by the birds afterwards; but if the birds attack it again, they become almost immediately comatose and can be destroyed. This remedy is expensive and not very effective. Is there any other drug you could suggest that would be less likely to be detected? Perhaps you know that I reported three years ago that these birds rendered isolation under the cattle plague regulations useless in some districts, as I proved beyond doubt they were the only means of communication between clean and infected herds under supervision, a mile or two apart. These birds I have never seen on the great herds of game on the open plains, but I have seen them on antelope and rhinoceros in the immediate neighbourhood of Masai villages, and herds of cattle; on the other hand, I have never seen the small egret on cattle, though often on rhinoceros and gnū."

Atmospheric Electricity.

IN NATURE of June 14 Mr. Wilson replies to the objections raised in my letter of March 29 to his explanation of the origin of atmospheric electricity. Before proceeding to consider Mr. Wilson's reply to my objections it may be well that the point at issue between us should be clearly defined, as Mr. Wilson, in my opinion, somewhat confuses it. Mr. Wilson says, "Mr. Aitken contends there is no such thing as dust-free air in the atmosphere." Now I certainly made no such statement, for the simple reason that I do not know whether such a condition exists to any extent or not, only a few cases being on record. What I did state was, "So far as our knowledge goes, it can hardly be said there is such a thing as dust-free air in our atmosphere, and the cases in which low numbers have been observed are so extremely rare that they can hardly have any bearing on phenomena of such widespread existence as atmospheric electricity, even though we suppose those few particles to be afterwards got rid of." I simply asked for a verdict of "not proven" against Mr. Wilson's theory. I think it will be admitted that it rests with Mr. Wilson, and those who think with him, to prove that the air is generally dust-free at elevations higher than ordinary cumulus and nimbus clouds, as without this dustless air the supersaturation necessary for condensation on ions is admittedly not possible.

Mr. Wilson discusses the question of the number of dust particles in the atmosphere from Mr. Rankin's Ben Nevis observations and my own at Kingairloch, and points out that practically dust-free air has been observed on Ben Nevis. Such is the case, but so far as I know dust-free air has been observed on only a few occasions, and such isolated instances have evidently no bearing on the case. Mr. Wilson then turns to my observations and says "the mean number of dust particles in a series of 258 observations, extending over nearly five years, amounting to 338 per c.c.; on one occasion the number was as low as 16 per c.c." The above statement, it must be clearly understood, refers to 258 of the tests made in the purest air, and is not the mean of all the observations. In the tables there are 688 observations for Kingairloch: of these I find there are 41 in which the reading was under 100, 341 were over a 100 but less than 1000 per c.c., whilst the remaining 306 observations were all over 1000 per c.c. The 16 per c.c. referred to by Mr. Wilson only occurred once. In the other years referred to the lowest figures were 38, 43, 67 and 205 per c.c. So that, as already said, the conditions represented by those low figures, such as 0 on Ben Nevis and 16 at Kingairloch, are so exceptional that they are not likely to play any part in phenomena so universal as atmospheric electricity.

Mr. Wilson, referring to the selected observations taken at Kingairloch on the pure air coming from the Atlantic, says: "Air coming from such a region can hardly be considered as abnormal. Moreover, such observations are necessarily made in air within a few feet of the ground; at a greater height it is likely to be less contaminated." Taking the last of these points first, an examination of the diagrams given along with the tables, from which Mr. Wilson made his extracts, will show that whenever the air became pure the readings low down and high up were nearly alike. This is shown by the curves in the diagrams for Ben Nevis and Kingairloch being nearly alike during these periods. Further, it may be seen from the curves that there was sometimes less dust at low than at high level when the air came from the Atlantic.

An examination of the tables from which Mr. Wilson took his Kingairloch figures easily refutes his assumption that the air of the Atlantic, as given in these tables, "can hardly be considered as abnormal." In the tables will be found the results of tests made in France, Italy and Switzerland. Observations were made at three places in France on the shores of the Mediterranean, at Hyères, Cannes and Mentone. An analysis of the figures for these places, made during visits extending over five years, shows that the lowest number observed was 725 per c.c., and of eighty-eight tests only ten were under 1000 per c.c., the others being all over 1000. At the Italian Lakes observations were made at Bellagio and Baveno. Many of these observations were made at elevations up to 2000 feet. In all, 188 tests were made: of these the lowest was 300 per c.c. On only thirteen occasions was the number under 1000, and 175 readings gave numbers over 1000 per c.c.

Perhaps it may be objected that all these Continental tests were made in low level polluted air. We shall therefore now examine the result of the observations made on the Rigi Kulm, given in the same tables. The top of the Rigi is 5900 feet above sea-level, but it has only the purifying effect of 4400 feet, as it is only about that height above the surrounding plains. During the tests, made on the visits during the five different years previously referred to, 259 observations were taken on thirty-two days, and the lowest number observed was 210 per c.c. Ninety-seven observations gave readings under 1000 per c.c., whilst the other 162 tests were all over 1000 per c.c. These tests, at both high and low level, give no support to Mr. Wilson's statement that the Atlantic air on the west coast of Scotland "can hardly be considered as abnormal."

Let me further support this point by reference to observations made by others of the air in different parts of the world. Prof. G. Melander, of Helsingfors, in his work, entitled "Sur la condensation de la vapeur d'eau dans l'atmosphère," gives the results of 268 tests made of the air at Salève, Biskra, Torhola, Loimola, Kristianssund and Grip. In all these 268 samples of air tested there were only five with less than 500 per c.c., and no low numbers were observed.

I now turn to the very interesting series of observations made by Mr. E. D. Fridlander and published in the *Quarterly Journal* of the Royal Meteorological Society, vol. xxii. No. 99, July 1896. In this paper Mr. Fridlander gives the results of his observations made during a voyage round the world from this country to America, across that continent to Santa Cruz Bay, from there across the Pacific Ocean to New Zealand, then to Australia, and homewards by the Indian Ocean, Arabian Sea and Mediterranean, visiting Switzerland on the way. On the western side of the Atlantic the numbers were high, being from 2000 to 4000 per c.c., though the vessel was far from land, its position being 55° 0′ N., 42° 11′ W. Lower numbers were obtained between Labrador and Newfoundland, the readings there being from 420 to 840 per c.c. Readings as low as 280 per c.c. were got in the Gulf of St. Lawrence. On the Pacific coast the lowest was 700 and highest 4500 per c.c. On the Pacific Ocean the lowest reading was 280 per c.c. and highest 2125. Few readings were obtained in New Zealand under 1000 per c.c. In the Indian Ocean the air seems to be rather purer than most places, or at least was so when the observations were made. Readings as low as 200 per c.c. were obtained, and they seldom were over 500. Tests made on only two days in the Arabian Sea gave a minimum of 280 and a maximum of 1375. One day's tests in the Red Sea gave from 383 to 490 per c.c. The result of two days' tests of the Mediterranean air gave a minimum of 875 and a maximum of 2500 per c.c. A result which agrees with that already given for the French coast of the Mediterranean.

Mr. Fridlander's tests of the air in Switzerland give results similar to those already referred to for the Rigi Kulm. At almost all the places the numbers were always over 1000 per c.c., though the observations were made at considerable elevations. But on the Riffelberg (altitude 7400), where Mr. Fridlander spent some days, the numbers varied from 225 to 4000 per c.c. On the summit of the Bieshorn (altitude 13,600) the lowest observation gave 140 per c.c., which, so far as I know, is the lowest number yet observed in Switzerland. When we compare the figures given by Prof. Melander and Mr. Fridlander of the dust particles in the air of different parts of the world with those obtained in the Atlantic air on the west coast of Scotland, we are forced to admit that the latter is abnormally pure.

The rate of fall of cloud particles as given by the calculations of Mr. Wilson seems to be much too rapid. He assumes that the air in which clouds are formed is always rising. This can hardly be said to be the fact. Suppose a large area of the earth's surface to be covered with cloud, forming a vast sea, such as one sometimes sees from the top of a mountain. It is evident that the air over all that area cannot be rising at any considerable rate, and yet the clouds will be seen to keep nearly the same elevation for hours. If the air be still, and if Mr. Wilson's calculations are correct, then the mountains ought to rise out of such a cloudy sea at the rate of nearly 500 feet per hour, a phenomenon which, I venture to say, no one has ever seen.

Mr. Wilson seems to think, though all the dust particles in cloudy air will not become centres of condensation, it is a matter of no importance, as he thinks the cloud will act as a perfect filter, by the descending cloud particles coming in contact with, and absorbing, the inactive dust particles. So that all particles that do not become active centres of condensation will be carried out of the air by the falling drops, and leave the air rising through the cloud particles dustless. He gives no evidence in support of this assumption other than the purification of dusty air in a closed vessel with wet sides. Now dusty air in a closed vessel takes a considerable time to become dust-free, and I think it may be contended that gravitation plays no inconsiderable part in the process, perhaps more than the wet sides referred to by Mr. Wilson. So far as my observations go, there is no evidence of any such powerful purifying effect in clouds. At least when making observations in old clouds, both at top and bottom of them, there were always observed a large number of dust particles, but whether any had been absorbed by the cloud particles or not it would be impossible to say. If any had been absorbed, certainly many were still free.

That clouds have not the purifying effect claimed for them by Mr. Wilson may be best shown by reference to the observations made on the Rigi Kulm on May 21, 1889 (*Proc.* Roy. Soc. Edin., vol. xvii. p. 193). On the morning of that day, when I left Lucerne on my way to the Rigi Kulm, the sky was covered with cloud, and when ascending the mountain the cloud was entered at an elevation of about 2000 feet below the top. On arriving at the top the clouds were still very dense, and remained so till the evening; afterwards they settled down to the level of the kulm, when a vast sea of clouds was disclosed stretching in all directions with the peaks of the higher Alps standing out like islands. Under these conditions the observations made on the top of the Rigi on that day were evidently taken in air just above the upper surface of a uniform stratum of cloud 2000 feet deep, where, according to Mr. Wilson, there ought to have been dustless air, yet the observations showed there were still 210 particles per c.c. Next morning the clouds still extended in most directions and were much thinner, and the number of dust particles had increased to over 800.

I may as a well here call attention to the fact that during the night the upper surface of the clouds had only settled down about 1000 feet. How much of this was due to the cloud particles falling through the air, and how much to evaporation, it would be hard to say. Probably evaporation played the principal part, as the clouds were now much thinner, and the evaporation probably took place from the upper surface, as in the morning the air on the Kulm was dry—the wet-bulb depression being as much as 6°, and a wind of some strength was blowing from the south-east. The rate of descent of the particles in this cloud was therefore much slower than the rate of fall calculated by Mr. Wilson.

Mr. Wilson, in criticising my remarks on the re-evaporation of cloud particles, says: "But all drops that have survived the great tendency to evaporate which accompanies the initial stages of their growth will surely continue to grow so long as the rate of expansion remains the same, or even if it be much reduced." Here again Mr. Wilson assumes that clouds are always rising. Now a great part of the life of a cloud, and the air in which the particles are carried, is spent in moving horizontally, and sometimes even downwards, and occasionally with but little movement in any direction; and it is during this stable condition that the opportunity is given for the re-evaporation of the smaller drops. Mr. Wilson points out that if a very slight proportion of the water in a drop were to evaporate, it would cool the drop and check the evaporation, a statement with which all will agree. But though the cooling may check evaporation, it will not stop it. The particles in a cloud are close together, and those condensing vapour and growing warmer soon part with their heat by radiation and by contact with the air, so that the heat lost by the evaporating particles is rapidly supplied to them by the condensing ones, and, as we shall see later, this exchange of heat takes place at a much quicker rate than one might imagine.

I do not think that practical chemists will agree with Mr. Wilson's statement that all the ammonia, nitric acid and other impurities, out of which the sun can manufacture nuclei, will be washed out of the air by the rain. The difficulty of removing the last traces of gases by washing is well known.

Are meteorologists prepared to accept that part of Mr. Wilson's theory which necessitates the formation of rain-clouds at an elevation of 7500 feet above the top of the ordinary cumulus and nimbus clouds? In other words, are meteorologists prepared to affirm that there are two distinct rain zones—one where the ordinary rain-clouds condensed on dust nuclei are formed, then over these clouds clear air for 7500 feet, above which the ion rain-clouds are formed? This upper ion-cloud must result in rain if the theory is correct, otherwise there will be no separation of the positive and negative ions. I leave it to the meteorologists to say whether rain-clouds have ever been observed at elevations of 20,000 to 30,000 feet—not above sea-level, but above the surface of the ground.

Mr. Wilson does not seem to think that my remarks on the rapid growth of cloud particles in supersaturated air have any bearing on the subject, and objects to my use of the term explosive in reference to the condition of supersaturated air. If I had known a better term I would have used it. Though supersaturated air is in a condition of equilibrium with itself, yet when nuclei are introduced into it there is at once a rapid rush of vapour molecules towards the condensing particles, and a rapid breakdown of conditions all round the nuclei, which seems to me not at all inaptly compared to an explosion—centripetally, of course. Mr. Wilson grounds his objection to the rapid growth of the ion-cloud particles in supersaturated air on the difficulty and slowness with which the condensing drops part with the heat developed by the condensing vapour. I shall not follow Mr. Wilson in his comparison of a condensing with an evaporating drop, as it is not easy to see the changes taking place in the latter, but will rather refer to an experiment which Mr. Wilson, and others who have experimented on this subject, must often have seen. Take a glass flask in which there is a little water, full of ordinary air, and provided with means of expanding the air in the flask, and either returning the air to the flask, or admitting filtered air. Go on repeating the process of expanding and cloud-making in the flask. After this has been done a number of times, the nuclei become fewer and fewer, and at last only a very few are left in the air. Every one must have noticed when making this experiment that the cloud particles are very small on the first expansion, and that they fall very slowly, almost imperceptibly, but that at the end of the experiment, when the last dust particles become nuclei, the water particles are large and fall rapidly like rain drops. At the beginning of the experiment, with plenty of dust in the air, there is almost no supersaturation, the nuclei being so close the tension is relieved as soon as it is formed. When, however, only a few particles are present, there are large spaces between the nuclei where supersaturation can take place, and it is by falling through this supersaturated air that the drops, when few in number, are able to grow so quickly and become so large. It therefore seems probable that something of the same kind will happen if ions were to become nuclei in supersaturated air. Whenever an ion becomes active it will rapidly grow to the dimensions of a rain-drop in the same manner and for the same reason that the dust-nucleused drops do in supersaturated air. These little drops evidently have a way of parting with the heat of condensation at a very much quicker rate than Mr. Wilson is disposed to admit.

It is this capacity for rapid growth in supersaturated air that makes it so improbable that ions can ever give rise to a cloudy form of condensation. To form a cloud a large number of them would require to become active at the same moment. But this is evidently not possible in a rising column of air. The ions which rise on the top of the ascending column will become active first, and by falling through the lower supersaturated air will grow with great rapidity and give rise to a rainy, but cloudless form of condensation.

There are some points connected with ions about which I think the readers of NATURE would be glad to have some information, and which I think Mr. Wilson, with the aid of the apparatus at his disposal, could give us. For instance, one would like to know (1) how long ions remain in air in an inclosed vessel, when both + and − ions are present; (2) when only + or − ions are in the air; (3) whether the presence of dust has any effect on the duration of their life. For practical purposes one would also like to know further (1) how many ions are generally in the air near the ground; (2) what amount of electricity they carry with them.

Finally, one would like to know how many ions will pass up through a cloud and escape at the top; as one would almost expect, these ions, with their electric charges, will be more likely to be cleared out of the air by rain than the dust particles, and whether both kinds are equally liable to be washed out by rain. If not, the inequality may help to explain some important electrical phenomena. JOHN AITKEN.

Ardenlea, Falkirk, June 27.

The Melting Points of Rock-forming Minerals.

IN connection with the abstracts of papers read before the Royal Dublin Society by Dr. J. Joly, F.R.S., and myself, given in NATURE for July 12 (p. 262), I might perhaps be permitted to draw attention to a few points. The same subject has been recently dealt with by Mr. C. E. Stromeyer (*Mem.* Manchester Lit. and Phil. Soc., vol. xliv. Part iii. No. 7, 1900) and by Prof. Sollas, F.R.S. (*Geol. Mag.*, July 1900).

In the first place it may be noted that the "melting point" of a substance under a definite pressure has a perfectly definite meaning. The "softening point," on the other hand, obviously depends on the magnitude of the distorting force with which the softness is tested, as well as on the other conditions of experiment.

It is an established fact that the melting points of a very large number of substances vary with the pressure. Bunsen, as far back as 1850, perceived the geological application of this phenomenon. In discussing the crystallisation of plutonic rocks, it is the melting points of the minerals under enormous pressures which really concern us. These pressures are probably sufficient to alter the melting points through several hundred degrees. There are then two ways open for us to ascertain these melting points. Firstly, we might determine them by direct experiment at the necessary large pressures; or, secondly, we might measure the melting points at ordinary atmospheric pressure and determine the rate of increase (or decrease) of melting point with increase of pressure ($d\theta/dp$). Considering the gigantic pressures with which we have to deal, it seems decidedly easier to adopt the second method. The agreement between the results obtained from the application of the thermodynamic formula

$$\frac{d\theta}{dp}=\frac{\theta(v_l-v_s)}{L}$$

(where θ = absolute melting temperature; (v_l-v_s) = the change of volume at the instant of melting; L = the latent heat in mechanical units) with the results of experiments (*e.g.* M. A. Battelli, *Journal de Phys.*, t. viii. p. 90, 1887), seems to justify the application of that formula to the case of the minerals in question, in the absence of direct experiment. It is true that the formula was deduced for a reversible system, and that no natural process is reversible. But a similar objection would hold against the application of any theoretical formula to the conditions obtainable in experimental work. In the present case it is only claimed for the formula that it will afford an approximate estimate of the melting points of minerals under large pressures; and after all, even direct measurement of such high temperatures as are involved is always attended with uncertainty. In order to apply this formula we require θ, (v_l-v_s), and L. The melting points of the most important minerals at atmospheric pressure have been determined by Dr. Joly and Mr. R. Cusack (*Proc.* Roy. Irish Acad., Ser. 3. vol. ii. p. 38; vol. iv. p. 399). A large part of the volume change on melting is, I submit, afforded us by the difference in density between the crystalline mineral and its fused glass. Now it is characteristic of amorphous substances to pass gradually and continuously from solid to liquid (*cf.* Preston, "Theory of Heat," pp. 270 and 286); and so it is highly probable that such a mineral glass will pass without sudden volume change into the liquid state, and it has, in fact, passed gradually in the inverse direction. It is not contended that any given mineral ever existed as a glass in the molten magma of an igneous rock, but only that it existed as a liquid.

In my paper, above referred to, I have shown how the "fusibility" of a mineral must be connected with its latent heat, and hence by a comparison of relative fusibility and melting temperature we may often deduce the relative latent heats of two minerals. Thus, for example, the "fusibility" of labradorite is 3 on von Kobell's scale, and its melting point is 1229° C., whereas orthoclase has a melting point of only 1175° C., but is much less "fusible," viz. 5 on von Kobell's scale. Hence I infer that the latent heat of orthoclase is decidedly greater than that of labradorite. Similarly, the latent heat of augite is less than that of orthoclase. But the volume-change on melting of augite is greater than that of orthoclase. Therefore $d\theta/dp$ is greater for augite than for orthoclase. It is thus possible to arrive at the order of melting points of minerals under the pressure of rock formation. If, after ascertaining this order, it is still found to be inconsistent with the order of crystallisation, as shown by microsopical examination, it may be necessary to examine the more complicated influences of solution, &c., on the crystallising points of the minerals.

In conclusion, I may point out that it must be a matter of extreme importance in measuring the melting temperature of quartz to make sure that the specimen used is pure, and in particular free from the alkalis. Messrs. Shenstone and Lacell (NATURE, May 3, 1900, p. 20) have found that rock crystal very often contains sodium and lithium, traces of which might be expected to lower the melting point. Further, it has long been known that quartz, with a density of 2·66, passes into the variety of silica with density 2·3 at a temperature below its melting point (*cf.* Fremy, *Enc. Chim.* 6, p. 142). And similar transformations are common among metals. Is it not possible then that the phenomena observed by Dr. Joly may have nothing to do with the fusion point of quartz, but are simply cases of molecular transformation at a temperature below the melting point? J. A. CUNNINGHAM.

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Observation of the Circular Components in the "Faraday Effect."

AFTER repeated attempts to determine the nature of the "Faraday effect," I have succeeded in observing that ordinary light, when passing from a surface into a medium in such a way